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TO WORK OR NOT TO WORK?
THE EFFECT OF HIGHER PENSION AGE ON CARDIOVASCULAR HEALTH

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Abstract

The study explores the possible unintended health effects of reforms aimed at making eligibility criteria for occupational retirement more severe. The causal link between retirement age and hospitalization for cardiovascular diseases is investigated in a large sample of male Italian retirees (N=94,521). Endogeneity is addressed by an Instrumental Variable identification strategy, in a quasi-natural experiment set-up. The instrument exploits the variation in pension age determined by the standardization of the labour market transitions, which induce workers born during the first months of the year to retire at an older age. The analysis is performed on a longitudinal dataset that combines several Italian administrative archives on pensions, working histories and hospitalization.

Results show a significant health detrimental effect of extended working life. A one-year delay in retirement increases the incidence of hospitalization for cardiovascular diseases (CVD) at 68-70 years old by 2.4 percentage points (p-value<0.01). Retirees who, during their careers, were lower income earners, mainly employed in the secondary sector and in manual occupations, are the groups paying the highest price for staying longer at work, as for them the impact of pension age on CVD is even higher. Sensitivity analyses show that results are robust to different model specifications; to the inclusion of career controls and to seasonality.

Key words: Retirement, Pension Age, Health, Cardiovascular Disease, Instrumental Variable, Ageing

JEL classification: I10 I14 J26 C36 J14

Introduction¹

Since the 1980s, there has been a remarkable increase in life expectancy across Europe, while the duration of working life has been decreasing, causing serious financial pressures on welfare systems. As a consequence, active ageing policies have been recently put at the core of Italian and European reforms' agenda. Within this framework, European countries have started to change radically their pension systems, by promoting the labour-market participation of elder people, restricting or eliminating possibilities for early labour-market exit, and increasing the age at which people are eligible for pensions, which is now set at around 67 years old in most European countries. Moreover, further increases are expected, as some states, and Italy among them, have linked pension age to changes in life expectancy, meaning that retirement age in these countries will soon grow well beyond age 67.

In this context of major transformations, an analysis of the actual costs and benefits associated with these reforms is crucial. It is of concern that the increase in retirement age may protract the exposure to adverse working conditions, work-related stress and occupational hazards, which may deteriorate health or pushing out of the labour market vulnerable workers categories via other social welfare programmes (i.e. long term sick leaves, disability pensions, unemployment). Since the desired outcome of such reforms is indeed not only a reduction in pensions' expenditures, but also the rise in the participation rates and the extension of working life, the target of having a healthy and productive workforce is fundamental, not only for ethical reasons, but also for the important economical and societal consequences that triggering health deterioration may have.

This paper investigates the potential health consequences of rising pension age. For this purpose, we have analysed Italian administrative data on social security and hospitalization records, which allows to measure very precisely both the outcome and the age at retirement, while controlling for important career characteristics. The health outcomes that we have

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considered in this paper are cardiovascular diseases (CVD), which represent the first cause of death and hospitalization (after child birth) in Italy, as well as worldwide (WHO 2014, Ministero della Salute 2014). CVD major risk factors have been shown to be influenced by working conditions, through widely recognized behavioural and pathogenic pathways (Marmot et al. 1997; Brunner 1997; Brunner et al. 2006; Perk et al. 2012).

The existing evidence on the relationship between retirement and physical health has so far provided mixed results. Several research design issues are a likely reason for this inconsistency across studies. The identification strategy that we have proposed, which relies on an instrumental variable approach that is novel to the health-retirement literature, together with the quality of the data analysed here, address most of these concerns. The chosen instrument has several nice properties. It exploits the exogenous variability induced by the month of birth on retirement age, which follows from the general standardization observed in most of the relevant labour market transitions. Thus, it captures the variability in retirement age *within* cohorts, while also allowing to test several hypothesis on its validity.

Our results show that the probability of being hospitalized for cardiovascular diseases at 68-70 years increases with higher pension age. Delaying retirement by one year raises the likelihood of hospitalization by 2.4 percentage points ($p < 0.01$). This age gradient is large relative to the 6.7 percent baseline hospitalization rate in the sample, as it corresponds to a growth of around 36%. This risk is not homogenous among individuals, as disadvantaged socio-economic groups suffer the highest increase in the probabilities of experiencing the health outcome (with an estimated increase growth up to 4.4 percentage points among retirees who were employed in low wage manual occupations, $p < 0.01$). Our analysis has focused on the 1937-1944 cohorts of male retirees, because, due to data availability, they are the ones for which the health outcome under study can be observed.

The rest of the paper is organized as follows. Section 1 provides a critical review of the literature more closely related to our study. Section 2 presents the data, discusses the definition of the treatment and the health outcome variables, and describes the analytical sample. Section 3 discusses the empirical strategy based on instrumental variable. Section 4 presents and discusses the findings relative to previous research. Finally, the last section offers concluding comments and policy implication.

1. Review of the Literature on Health and Retirement

Based on the existing literature, from a theoretical perspective it is not clear what should be the effect of retirement on health. Retirement is a major life transition, with high stressful potential because it entails a disruption of own social role, habits and identity (Atchley 1976). Postponing retirement may reduce the age-related decline of health, due to a greater exposure to cognitive and physical stimuli, social relationships and other intrinsic benefits associated to work (Jahoda et al. 1981; Warr 1987). Moreover, according to the health capital accumulation theory (Grossman, 1972), the incentives to invest in health cease to exist with retirement, because there is no more the need to keep work productivity high. On the other way around, retirement makes more leisure time available, which could be beneficial for individuals' well-being. Indeed, having more free time available reduces the marginal cost of health investments. Moreover, with retirement there is a relief from the exposure to physical and psychological demands associated to work, which might be harmful for health (Karasek 1990, Singrist 1996). Hence, relying solely on the existing theories, it is not possible to give a definitive prediction of what should be the relationship between retirement and health.

As we discuss below, also the available empirical evidences have not been fully conclusive so far. In this respect, differences in research designs, as well as in the definition of the exposure and the outcome variables, may explain why results are often puzzling when compared across studies. Depending on the health outcome that is analyzed, the association between retirement and health is different. A systematic review of longitudinal studies has found strong evidence that retirement has beneficial effects on mental health (van der Heide et al., 2013). Research on cognitive abilities has provided almost unanimous evidence of an acceleration of the decline in cognitive abilities following retirement (Dave et al. 2008; Rohwedder et al. 2010; Bonsang 2012; Mazzonna et al. 2012 and 2016). Instead, results are more conflicting for physical health (for a systematic literature review, van der Heide et al., 2013). This might be due to the longer latency required for a physical health outcome (e.g. CVD and other chronic diseases) to display changes, but also to the fact that some study design issues, in the case of physical health, are more relevant.

A first source of inconsistency across studies regards the definition of the exposure variable. It is common to operationalize retirement as a dichotomous treatment, with little or no

attention on retirement timing and intensity.² However, retirement is potentially more complex than a binary treatment, which, by construction, displays its effect in a “one off” manner regardless the age of the exposure. Moreover, different reasons for being “not in paid employment” (e.g. inactivity, voluntary, involuntary and health driven retirement) are often pooled together. This conceptualization, often driven by data constraints, might create a misclassification problem, since it mixes the effects of “heterogeneous exposures” on health. Another important issue is the type and quality of the job performed before retirement, as emerged in a few studies which have found that retirement is associated with better physical and mental health only among workers in low quality jobs (Matthews 2014; Westerlund et al. 2009; Kalousova et al. 2015).

Finally, yet importantly, identifying the causal effect of retirement on health is problematic due to the potential endogeneity of retirement. Retirement can be driven by health conditions, as it is well established that people in bad health tend to retire earlier. Thus, observing poor health after retirement might not be the effect, but the cause of retirement (Behncke 2012). The studies that have attempted to identify the causal effects of retirement on health have used different strategies to overcome this empirical challenge, often providing contradictory evidence. Limiting our attention to studies on physical health outcomes (self-assessed general health, mortality, chronic diseases), those that adopted a longitudinal design and controlled for confounding factors have generally found negative health effects of retirement (Morris et al., 1994; Dave et al., 2008; Bamia et al., 2007; Moon et al. 2012; Wu et al. 2016). Instead, studies that adopted a quasi-natural experiment set-up (IV, RDD) have generally found that the effect of retirement on health is either beneficial (Coe et al. 2011; Inlser 2014; Eibnich 2015 Bound et al. 2007) or null (Hernaes et al. 2013; Johnson et al. 2009; Behncke 2012).

In this paper, we have overcome several problems and limitations which typically arise when studying the health-retirement relationship, contributing to the literature in several aspects. We have studied the impact of the *age at retirement* on health, as rising minimum pension age is the main objective of most of the recent pension reforms. To solve the problem of

2 Among the exceptions, there are the works by Harnaes et al. (2013), who has focused on the impact of retirement age on mortality, and by Mazzonna et al. (2012, 2016), who has looked at the effect of time spent in retirement on cognitive abilities.

endogeneity, we have adopted a novel approach, exploiting the exogenous variability in retirement age induced by the month of birth. To the best of our knowledge, this is the first time that birth month is used as instrument in the retirement-health literature. This approach has several advantages, which are discussed in more detail in Section 3. By using Italian administrative data on social security and hospitalization records, we have been able to measure precisely both the outcome and the age at retirement, while controlling for important career characteristics. Finally, we have considered occupational retirement alone, limiting the problem of mixing the exposition variable, which usually arises from pooling together different reasons for retirement.

2. Data and variables definition

This study is based on WHIP&Health, a longitudinal database on work and health histories in Italy built upon administrative records drawn from the National Social Security Institute (INPS) and the national archive on Hospital Discharge Data (Bena et al. 2012). The INPS insures approximately 23 million workers, representing more than 80% of the workforce in Italy. By means of a unique individual identifier, we linked a 7% random sample, taken from the social security archive on the whole careers of employed and self-employed private sector workers, to the national archive on hospitalization events registered in all private and public Italian hospitals.

The treatment

Our treatment variable is “retirement age”, i.e. the actual age at which the individual starts officially receiving an old age pension or a seniority pension benefit from the Italian Social Security Institute (INPS). This is operationalized as

$$Pension\ age = (Date\ of\ First\ Pension\ Flow - Date\ of\ Birth)/365$$

We adopted an administrative definition of retirement, and we focused only on occupational retirement, in the attempt of overcoming some of the limitations previously discussed. Indeed, defining retirement as anyone who is not in the paid labour force has the main pitfall of mixing occupational retirement with unemployment, inactivity, and disability pension,

which may all have independent effects on health³. Moreover, an administrative definition of retirement has the advantages of not suffering from the possible biases typical of self-reported measures (recall bias, justification bias, and adherence to social norms) (Currie et al. 1999).

A pure administrative definition of retirement however has also limitations. Retirement is a more complex life and labour market transition than starting receiving a pension flow. The borders of this transition are not always sharp, and retirement may occur through an array of heterogeneous “bumpy” (Contini and Leombruni 2006) or “gradual” pathways (Bloemen et al. 2016; Eurofound 2016). Workers may not enter into retirement abruptly, but rather through an intermediate transition into other programmes, e.g. disability and unemployment benefits, or after reducing gradually their labour participation.

The health outcome

The archive on hospital dismissal is based on the systematic collection at the national level, by the Italian Ministry of Health, of the regional archives of hospital admissions (*Schede di Dimissione Ospedaliera*, SDO). This archive contains information for years 2001-2014 about all patients admitted to public or private hospitals in Italy, including the diagnosis at the moment of the discharge from the hospital, codified according ICD disease codes (ICD-IX)⁴.

The present paper investigates the risk associated to retirement age and cardiovascular diseases (CVD), a group of diseases that includes myocardial infarction and other acute or chronic forms of coronary heart diseases (ICD-IX 410–414) and stroke (ICD-IX 430-438). The outcome is operationalized as an incidence. That is, it is a dichotomous variable defined on the population of individuals who were never hospitalized for cardiovascular diseases

³ In fact, the presence of a disabling health condition is a formal requirement for a disability pension; unemployment is a major stressful life event linked to inflammatory processes leading to ill-health (Hughes et al. 2015), increased risk of depression (Paul and Moser, 2009), all-cause mortality (Roelfs et al. 2011) and coronary heart disease (Ardito et al. 2016). Moreover, bad health is among the factors favoring early withdrawal from the labour market (McGarry 2004; Ranzi et al. 2011).

⁴ ICD-9-CM is the ninth version of the International Classification of Diseases, adopted in 1975 by the World Health Organization. This is the classification adopted by Italian Ministry of Health to codify diagnosis and procedures included in the Hospital Discharge Data (*Scheda di Dimissione Ospedaliera*, SDO). For more detail visit the page:

http://www.salute.gov.it/portale/temi/p2_6.jsp?lingua=italiano&id=1277&area=ricoveriOspedalieri&menu=classificazione

between the age 64 and the age 67. It takes value one if the individual had a first occurrence of CVD hospitalization at 68-70 years old, and zero if the individual had no CVD in that age range. In this way, we keep constant, among all the individuals and among all the birth cohorts, the age-window to observe the first occurrence of the outcome (at 68-70 years old) and the “baseline” window to drop out individuals who already had a previous CVD (at 64-67 years old). A caveat with this data source is that we only observe hospitalizations for the years 2001-2014; hence, we cannot exclude the possibility that a minority of our first CVD events are recurrent ones, for it might be that a small proportion of individuals had a CVD before age of 64.⁵ Nevertheless, under this data limitation, this way of defining the outcome offers the advantage of not generating any correlation between birth cohorts and the probability of having the outcome, which is instead induced when using windows of observation of different length for different cohorts (Herneas 2013).

CVD hospitalization is a health indicator based on the assessment of hospital physicians, thus it is free from some of the typical biases associated to self-reported measures, like the “justification bias”. Indeed, previous contributions have documented that unemployed, retirees and inactive individuals tend to justify their exit from the labour market emphasizing the importance of their health conditions, even if the decision was to some extent motivated by financial considerations or by a relative preference for leisure (Bound 1991; Kapteyn et al. 2007; Kerkhofs et al. 1995). Moreover, CVD hospitalization is a severe health outcome, reducing the potential biases associated to care seeking behaviors and social gradient in the utilization of medical care (Van Doorslaer et al., 2000).

Nevertheless, also a measure based on administrative records for CVD hospitalization is not free of limitations. There exists the possibility that a number of CVD events have not been recorded, as it happens for example with the so-called silent or unrecognized myocardial infarction⁶ or when the person dies before hospitalization. Moreover, there might be some

5 Based on our database of CVD hospitalizations, only around 5% of recurring episodes happened after four or more years from the previous hospitalization. This figure was computed using all (first and subsequent) CVD events occurred in Italy between 2007 and 2014.

6 An unrecognized myocardial infarction is defined as asymptomatic or associated with minor or unusual symptoms. Estimates based on a recent literature review suggest that up to 6% of the general elderly population

quality difference between hospitals that may translate in a more or less rigorous compilation of hospital discharges records. However, although some possibility of misclassification yet exists, Italian hospital discharge data have been proved to be a valid and reliable tool for public health surveillance (Leone et al. 2004; Palmieri et al. 2007; Leombruni et al. 2010). Furthermore, the instrumental variable design prevents the problem of a systematic measurement error that may bias the estimate.

Sample selection & summary statistics

Since we are interested in analysing the impact of retirement age on the probability of being hospitalized for CVD at 68-70 years old, we selected from the social security archives, only males born in Italy in years 1937-1944 who received an occupational pension between 1985 and 2012. In this way, we eliminated all the persons who received any disability or early-retirement pensions ($N= 106,934$)⁷. We then further selected who at the age of 68 was retired (>99%), alive (>90%) and with no CVD in the previous 4 years (>88%). We obtained a final sample of 94,521 retirees, which constituted our first sample of analyses, described in the first column of Table 1. The sample is rather representative of the general population of Italian private sector retirees, as confirmed by comparing our data with official statistics obtained on the census of retirees. For example, over all the occupational pensions paid to men retirees at the 31st December 2012, the average pension benefit was 1,008 euro (in 2005 prices), the average proportion of seniority pensions was 70% and the proportion of individuals insured in the Employee funds was 63.3%.⁸

For a subsample of these retirees, we could observe also their late career, starting from the year 1985, hence for retirees employed at 1985 it was possible to investigate the association between pension age and CVD controlling also for a set of career variables. This subsample of retirees constituted our second sample of analysis ($N= 50,854$) and is described in the second column of Table 1. They are certainly not a random sample of the whole retirees, as

have experienced one (Valensi et al. 2011). However, prevalence vary widely between study populations. For example, silent myocardial ischaemia affects 20–35% of diabetes patients (Rydén et al. 2013).

⁷ In Italy, early retirement schemes have been widely used during the 80s and the 90s, as a legal instrument offered to manufactory firms under financial constraints to favour turnover of older workers, hence early retirement was essentially used to replace unemployment.

⁸ Authors' elaborations based on (INPS 2013, p. 243, 261 and 262).

they were selected for being “in employment”. Therefore, they are likely to have different characteristics with respect to the retirees who were already out of employment in 1985.

In the last column of Table 1, we reported the results of the hypothesis test that the two samples means were equal between the sample of individuals with job spells after 1985, and individuals without an observable work history. It emerges that retirees who were employed after 1985 were a few months younger than the retirees who stopped working before 1985, they had greater labour market attachment, as shown by higher levels of contributions and pension benefit. They also retired more often via a seniority pension, a type of pension stream for which the most relevant eligibility criteria is years of contribution rather than age, and they retired on average two years earlier. However, importantly, both the incidence and the prevalence of CVD were statistically the same in the two groups. All the subsequent analyses will be based on these two final samples, denoted from now on as “All retirees” and retirees “Employed after 1985”.

Table 1 Description of the Final sample

		All retirees	Employed after 1985	Test
		mean (sd)	mean(sd)	Equal means
Demographics	Age (at 2001)	60.14(2.29)	60.06(2.29)	-0.175***
	Birth region: North (%)	0.44	0.46	0.041***
	Centre (%)	0.16	0.16	-0.0008
	South (%)	0.40	0.38	-0.04***
Characteristics at Retirement	Pension Age	59.26(4.86)	58.21(4.77)	-2.281***
	Pension year	2000(5.16)	1999(5.07)	-2.073***
	Employees fund (%)	0.60	0.88	0.614***
	Seniority pension (%)	0.72	0.79	0.159***
	Contributions (Yrs)	31.76(9.17)	33.91(6.34)	4.667***
	Monthly Pension (€,real)	1,071(712.61)	1,396(744.02)	703.29***
CVD	Incidence at 68-70	0.07	0.07	0.0007
Career characteristics	Manual Occupation		0.68	
	Contributions at 52 (Yrs)		28.72(7.28)	
	Annual wage (€,real)		23,481(16,360)	
	Services sector		0.68	
	Industry sector		0.31	
N. Observations		94,521	50,854	

*Note: Pension benefit and monthly wage are real, at 2005 price. T-test for the hypothesis that the subsamples of retirees who were and who were not employed after 1985 have equal means. The difference between the samples means and the significance level is reported. Legend: * $p < .10$, ** $p < .05$, *** $p < .01$*

3. Empirical strategy

Endogeneity issues

We want to study the effect of age at retirement x_i (continuous) on a measure of health y_i (CVD hospitalization), controlling for a set of covariates W_i . In particular, we want to estimate the following linear model

$$y_i = \beta_0 + \beta_1 x_i + W_i + u_i \quad (1)$$

However, there are several reasons why retirement age might be correlated with the unobservable error term, u_i , producing biases of different signs.

The problem of simultaneous causality must be remarked at the outset. If prior poor health is positively correlated with successive incidence of health outcomes, and negatively correlated with actual pension age, since it induces individuals to anticipate retirement, this imposes a negative bias to our coefficient β_1 . This type of bias, usually denoted as a “healthy worker effect”, was very common to most of the literature during the nineties, yielding a misleading picture of how health evolves with retirement. Indeed, ignoring the fact that workers in better health are more likely to survive and to continue to work, and simply comparing the health of retired against that of workers, has usually led to the finding of worst health among retirees than in the employed population (Maimaris et al., 2010; Shim et al. 2013; Ranzi et al. 2013 for Italy).

A second identification problem is given by the fact that the association between retirement age and bad health might reflect a purely spurious association, if an unobservable third factor correlates with both health and the time of retirement. For example, people with economic constraints might prefer to work longer, and at the same time, their economic disadvantage is probably correlated with bad health, according to the well-known socio-economic gradient of health (Marmot 2005), leading to a positive bias of the estimated coefficient.

A third problem is that age of retirement may be measured with errors, since it may occur that some periods before retirement are spent out of the labour market voluntarily, waiting for an age eligibility criteria, or in various unemployment, partial unemployment or disability benefit schemes. This would result in an “actual” retirement age that is lower, and simply looking at the official date at which a person starts receiving a pension flow might lead to a measurement error. The error could be correlated with health if, for example, people with some medical conditions tend to exhibit higher unemployment or inactivity before achieving the pension eligibility, which means that the estimated coefficient would again be biased up.

In conclusion, because of these opposing signs of bias, it is not possible a priori to define a direction to the overall bias that would affect the estimated retirement age coefficient by adopting an OLS model. However, since it is common among studies where there were few

or no controls for endogeneity to find out that retirement was associated with worsening health conditions, this might suggest a possible predominant role of a downward bias.

Two-Stage Least Squares Instrumental Variable

In order to estimate the causal relationship between pension age and the subsequent risk of CVD, we implemented a Two-Stage Least Squares Instrumental Variable (TSLS-IV) estimation strategy, exploiting the variation in the retirement age induced by birth month. The IV strategy relies on the discontinuity in retirement age that arises from an exogenous factor that does affect CVD only through pension age. The proposed instrument is based on birth month. To the best of our knowledge, it is the first time that this kind of instrument is adopted in the literature on the health effects of retirement.

The instrument, which is operationalized as a variable taking value one for individuals born in the first four months of the year, has several nice properties. To begin with, it is relevant, in the sense that it influences positively and significantly the age of retirement. As a preliminary graphical evidence shows, there is an almost perfect linear (unconditional) correlation between retirement age and month of birth, with January being the month with highest pension age (Figure 1). This association is confirmed also once we condition on year of birth (as well as in the fully adjusted IV model, Table 1). Among all the cohorts (but with the partial exception of the 1940 one), individuals born in the first months of the year have the highest retirement age. In Figure 2 we have combined the birth months by groups of four (*i.e. quadrimester*). It emerges a clear “zig zag” pattern, with individuals born in the first quadrimester (Q1) that have consistently higher retirement age than the individuals born in the next months.

We suggest that the main mechanism inducing this positive relation between retirement age and birth month is the high level of “standardization” of the main labour market transitions faced by individuals throughout the life course, e.g. entry in school, school-to-work and work-to-retirement transitions. Indeed, typically school entry and leave dates (September and June), or job contracts start (January) and end (December) dates are the same, irrespective of the individual month of birth. While education timing is related to the “school year”, firms rely on the “fiscal year” for the activation and termination of contracts, which in most of the

cases tend to be opened in January and closed in December (Leombruni and Quaranta, 2002). This implies that individuals born earlier in the year begin the school, enter in the labour market and retire at an older age. Job contracts tend to have typically standardized timing, as they tend to start at the beginning of the calendar year and to expire at the end of it following the fiscal year, which in Italy starts in January and ends in December. This produces in turn further tendencies toward a standardization of seniority contributions among members of the same cohort. Thus, individuals born earlier in the year will tend to reach the minimum contribution requirements for occupational pensions at an older age, with respect to individuals born later in the year.

Figure 1. Pension age by month of birth

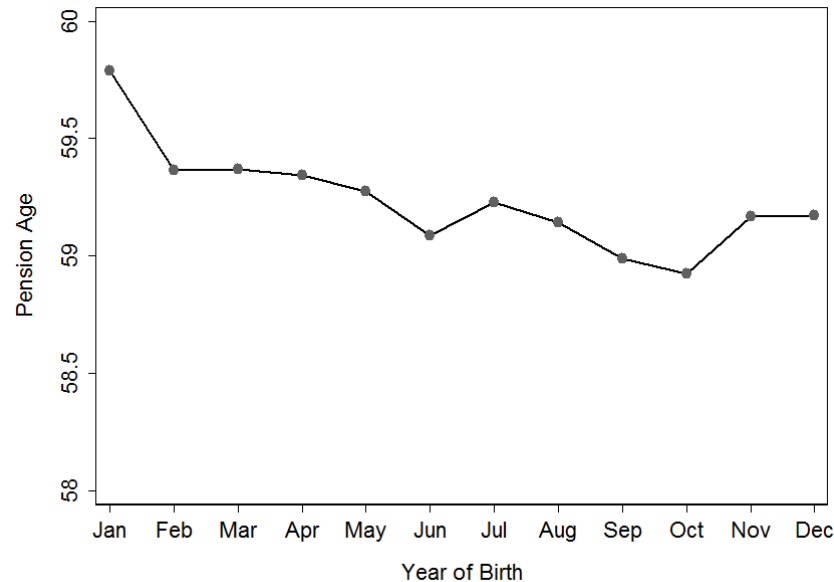
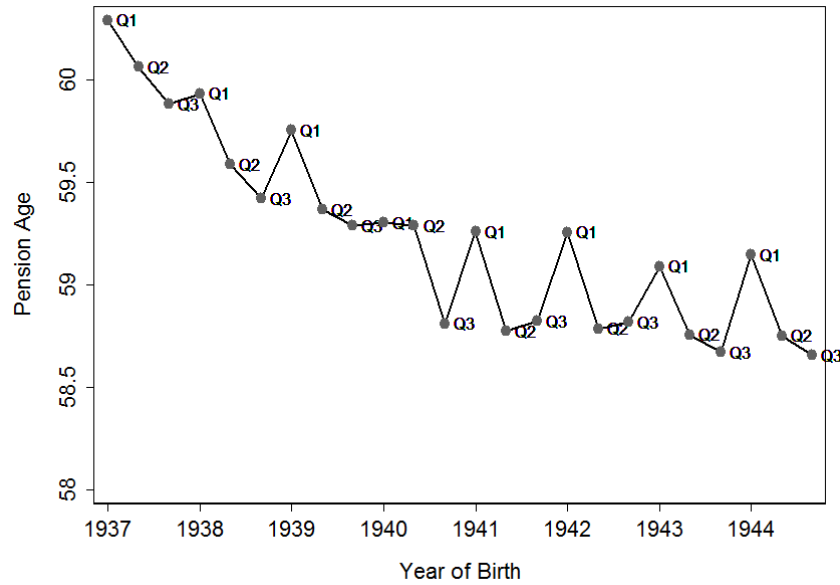


Figure 2. Pension age by year and birth month (grouped by quadrimesters)



A second advantage of the chosen instrument is that it allows capturing the (exogenous) variability in pension age *within* cohorts. Traditional approaches, which exploit changes in pension eligibility thresholds as a source of discontinuity in the probability of retirement, typically rely on parallel trend assumptions in health among cohorts, which may be difficult to justify in the context of improving health conditions across time. Moreover, the problem of heterogeneous individual responses to the policy change provides a further reason why, in the present context, the chosen instrument is to be preferred over the standard IV or RDD research design. As mentioned, in the literature that addresses the problem of causality, it is quite common to consider as exogenous the discontinuity in the probability of retirement induced by changes in the eligibility rules. However, one of the crucial assumptions for a correct and interpretable identification of the LATE, both in IV (Imbens and Angrist 1994) and RDD (Hahn et al. 2001) study designs, is monotonicity. Monotonicity requires that when the age eligibility requirements are, for example, postponed, either all the workers will tend to anticipate or they will tend to postpone retirement (“no defiers” condition). Since the effect of retirement on physical health is probably not homogeneous in the population, due to the important differences in terms of career exposures, socio economic, individual and family resources among individuals, the plausibility of the monotonicity assumption, although not

often discussed,⁹ is a fundamental one. Indeed, in case of its violation the estimate of the average treatment effect can be biased (Imbens et al. 1994).

In the Italian context, there are reasons to believe that the changes occurred in eligibility rules across time have been characterized by high levels of non-compliance. Indeed, in Italy the pension reforms implemented throughout the 1990s, which are the relevant ones for the years covered by our data, have targeted different portions of the population at different points in time, leaving individuals non negligible time and freedom to adjust their labour supply. The pension reform initially raised the minimum old age from 60 to 65, and only a few years later the seniority requirements were increased from 35 to 37 years of contribution, and even later a minimum binding age for seniority pension was introduced (INPS 2013). This fragmented implementation have induced anticipation and escape mechanisms among portions of the Italian elderly workforce (e.g. Santoro 2006; Arpaia et al. 2009) which undermine the plausibility of the validity of an instrument based on such policy change. To overcome this important limitation, we propose to use a different instrument, by relying on a source of variation that is persistent and similar across cohorts. Moreover, for the generations included in our analysis (1937-1944), the relevant pension thresholds have always remained constant, so that heterogeneities in the responses to policy changes are not a source of concern for our results. Finally, the evidence provided by Figures 1 and 2 suggests that the sign of the correlation between birth month and retirement age is similar across all cohorts. Therefore, monotonicity seems to be a safer assumption in the present context.

Nevertheless, the chosen instrument is not free from other potential limitations. In particular, we should be worried by the fact that some unobservable factor may be correlated with both the month of birth and physical health. This could be the case if, for example, some health problems were correlated with the season of birth, or if the choice of the period of birth was correlated with socio-economic characteristics of the family. However, Section 4 presents two tests that control for these hypotheses, both supporting the main findings of the paper.

⁹ Among relevant exceptions, it is worth mentioning the work of Bertoni et al. (2016), who have assumed the monotonicity condition implicitly satisfied in their analysis and Mazzonna and Peracchi (2016) also assumed no anticipation effects, although, as we will discuss, evidence for Italy suggest that the opposite is true.

4. Results

Table 2 shows the IV-2SLS estimates¹⁰ of the effect of retirement age on the incidence of CVD hospitalization. Each column represents a separate regression, with a different set of controls. For each regression, only the estimated coefficient of pension age is reported, together with some relevant statistic, i.e. the average incidence of the outcome, the sample size and, as a diagnostic of the quality of the IV procedure, the Kleibergen-Paap (2006) F-test. This latter test is used to measure whether the instruments are weak, i.e. whether the instruments are only poorly correlated with the endogenous regressor. The null hypothesis is that the effects of the excluded instruments are jointly equal to zero once that the endogenous regressor is conditioned on the controls included in the model. According to a standard rule of thumb proposed by Staiger and Stock (1997), an instrument should be considered weak if the first-stage F-statistic is less than ten. However, according to the F-test critical values calculated by Stock and Yogo (2005) for one single endogenous regression, we should consider as suggestive of poor performance also any F-test values lower than sixteen.

In the first column, the analysis is run over the whole sample of retirees, and the included controls are dummies for region and year of birth. The IV estimated a highly significant ($p < 0.01$) coefficient of 0.029, meaning that for an additional year of age at retirement, the incidence of CVD hospitalization at 68-70 years old increased by 2.9 percent points. The F-test is greater than 30, suggesting that our instrument is performing quite well. In the subsequent column, we replicated the same specification on the subsample of retirees who were still employed at 1985, which implied a reduction of slightly less than 50% of the observations. The point estimates resulted higher in size, but the 95% confidence interval almost perfectly overlapped, suggesting that the two are not significantly different.

In the last column, considering the sample of retirees still employed at 1985, we added the career controls variable, which likely correlate with both pension age and CVD incidence, so that their omission might produce an omitted variables bias. We included a full set of dummies for the sector of activity, the occupation, the total weeks of seniority contributions

¹⁰ Analysis are performed with Stata11 using the command `ivreg2` for the estimation of the IV-2SLS model (Baum et al. 2010).

paid up to 52 years old (measured in year-equivalent terms) and the average annual wage, computed using all the job spells from 1985 to retirement. The inclusion of career controls did not alter the quality of the result and confirmed that retirement at older age is associated significantly to an increase in the incidence of CVD hospitalization, by around 2.4 percent point. The point estimate only reduced marginally in size with respect to the other specifications, but gained more precision as showed by smaller confidence intervals, mainly thanks to the important improvement in the instrument performance in the first stage regression, as shown by an Kleibergen-Paap F-test equals to 122.

Table 2 Effect of pension age on CVD hospitalization incidence at 68-70 years old (TSLS)

	All retirees b (95%ci)	Employed after 1985 b(95%ci)	Employed after 1985 b (95%ci)
	0.029*** (0.009,0.049)	0.032** (0.007,0.056)	0.024*** (0.007,0.041)
Instrument	Q1	Q1	Q1
F-test	38	29	122
Observations	94,521	50,854	50,854
CVD incidence	0.066	0.066	0.066

*Notes: Each column represents separate regression. Model 1 and 2 include year and region of birth dummy variables. Model 3 adds controls for, log of wage, years of contribution and dummy for sector of activity. Robust 95% CI in parentheses. Legend: * $p < .10$, ** $p < .05$, *** $p < .01$*

Since the last specification performed better, and it controls for important individual characteristics, we adopted it as our preferred baseline regression. Using the same sample, we run a series of sensitivity checks aimed at testing the robustness of our point estimate, together with the validity of our instrument. The results of these tests are reported in Table 3, where the first column reports again the result of the baseline model for simplicity of comparison (column 1). As a first robustness check, we changed the probability model, relaxing the assumption of linearity and fitting an IV probit model to the data (column 2). In this case, the marginal effect of pension age on CVD is 0.028, suggesting that higher pension age is associated to an increase in the probability of hospitalization for CVD of 2.8 percent points, an amount very close to the baseline estimated effect.

Table 3 Robustness analysis of the Effect of Retirement Age on CVD Hospitalization (TSLS)

	(1) Base	(2) IV Probit	(3) Overidentified	(4) Seasonality	(5) Discontinuity sample
	0.024*** (0.007,0.041)	0.028*** (0.075,0.258)	0.027*** (0.012,0.042)	0.031*** (0.011,0.050)	0.031** (0.006,0.055)
Instrument	Q1	Q1	Q2, Q3	Q1	Q1
F-test	122		74	92	53
J-test (p)			0.435		
Observations	50,854	50,854	50,854	50,854	10,168

Notes: Each column represents separate regression. All regressions include year and region of birth dummy variables, log of wage, years of contribution and sector of activity dummies. Robust 95% CI in parentheses. Legend: * $p < .10$, ** $p < .05$, *** $p < .01$

In the third column, we exploited the possibility of using two instruments. Instead of the indicator for being born in the first four month of the year, we used separate dummies for individuals born in the second and the third quadrimesters of the year. The model confirmed again the positive and highly significant pension age coefficient. According to this specification, the incidence of CVD was increased significantly by 2.7 percent points for any additional year of work. Since in this case we have used two instruments for one endogenous regressor, this overidentification allowed us to check, at least to a given extent, the exogeneity of our instruments, by means of the Sargan-Hansen J-test test (1985). This is a test of overidentifying restrictions, which is consistent also in the presence of heteroskedasticity. The joint null hypothesis is that both the instruments are uncorrelated with the error term, under the assumption that at least one of them is actually exogenous. Hence, we want to accept the null to gain confidence that the instruments are valid. The p-value of the J statistic, which is reported in column 3, is very high (0.435), suggesting that the null hypothesis of exogeneity cannot be rejected. However, we should stress that the J test is valid only under the assumption that at least one of the instrument is exogenous. Additional evidences in support of the hypothesis that the instruments are indeed exogenous is provided by the next tests, where we address explicitly one of the main concerns when using birth month as instrument, i.e. seasonality.

One standard critique to the birth month instrument is that it is indeed not exogenous, because it might capture the effects of seasonality in terms of exposure to different

environmental and climatic conditions (Buckles et al. 2013). Moreover, also mothers' characteristics may differ, because of sorting based on preferences for giving birth in certain periods of the year. It is indeed well established that early life conditions (Gluckman et al. 2008; Kelly et al. Bartley et al. 2012), as well as climate at birth, may have long lasting effects on future health (WHO 2012; Xu et al. 2012; Pezzoli et al. 2016). On this respect, obviously birth month correlates with average climate temperatures, but it does do in a reverse u-shaped manner (see Figure 3), which is very different from the linear relationship that we observed between pension age and birth month in our data. While January and December correspond to the two extremes of the January-December gradient in retirement age, there is not correspondence of such discontinuity in the temperatures, as January-December are actually contiguous and are among the coldest months of the year.

To formally test whether and how seasonality might bias our estimate, we perform two analyses. First, in column (5), we include among the controls the average monthly temperature at birth taken from the Global Summary of the Month database (NOOA 2016). Only the city of Milano was available for the years of interest (1937-1944).¹¹ Thus, we decided to consider this variable as a proxy for Italy, in the absence of time series data for all the other regions. Although not very precise, the temperature data point is monthly- and year-specific, and it provides an approximation of the evolution of monthly temperatures at the time the individuals of our sample were born. The relevant average monthly temperature was divided in tertiles and included as dummies¹². The estimated coefficients increased in size and significance ($\hat{\beta}_{seasonality}^{IV}=0.031$, $p<0.01$), improving the estimation precision with respect the baseline specification. The fact that the inclusion of temperatures increased the coefficient suggests that if any, the baseline estimation was downward biased¹³.

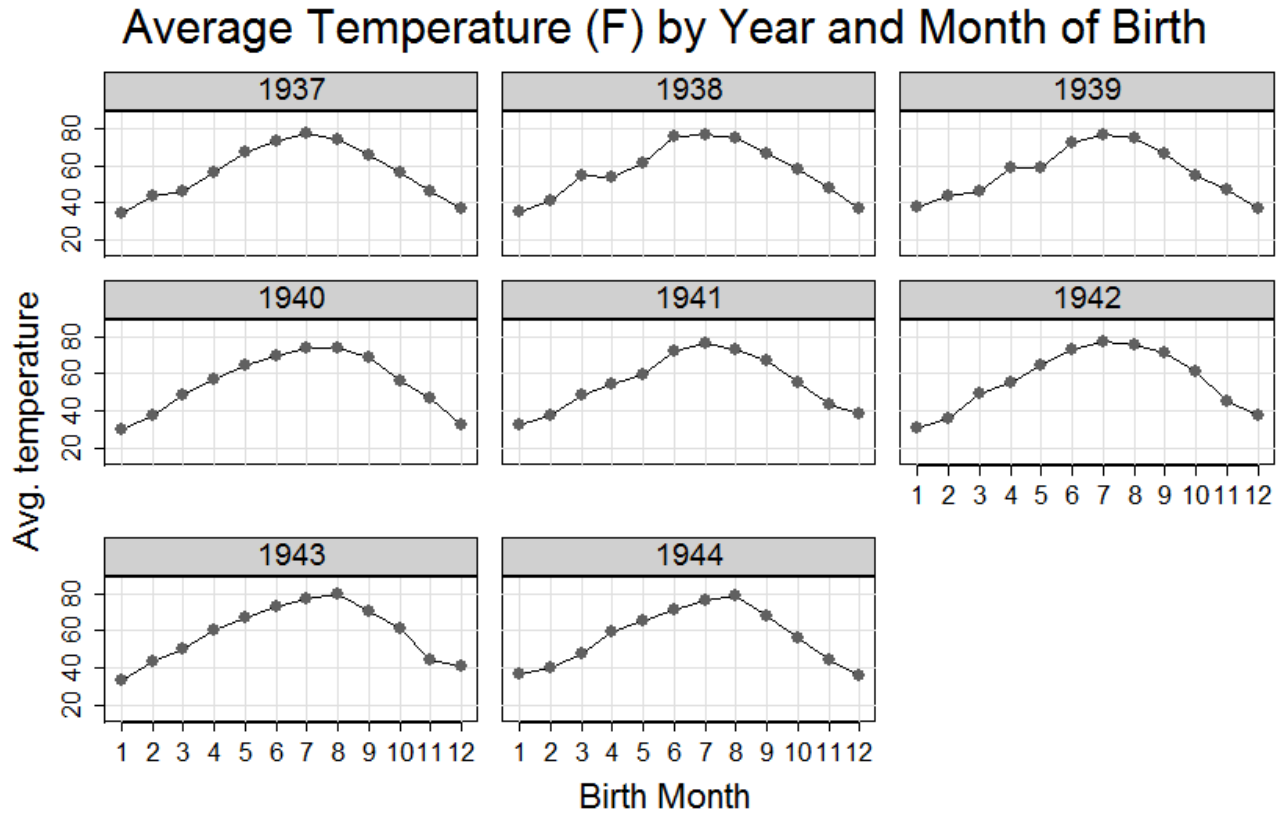
11 The database is very rich in data and geographical coverage, as for most of the countries in the world it collects a wide range of meteorological data. For Italy, only from more recent years (from 1950 onwards) all the major geographical areas are represented.

12 The inclusion of the temperature as a continuous variable gave qualitatively the same result.

13 The estimated coefficients of the temperature dummies suggested that, holding constant all the other factors, individuals born in the warmest months had higher likelihood of CVD hospitalization (estimates beta for the second tertile: 0.007, $p<0.05$; for the third tertile: 0.005, $p<0.13$).

Last column of Table 3 presents another way of netting out the possible confounding effect of seasonality. Following a common practise in the birthdates literature (Beuchert et al. 2016; Ponzo and Scoppa 2014; Elder and Lubotsky 2009 and Puhani and Weber 2007), we exploited the discontinuity in retirement age restricting the sample only to individuals born in the months of December and January. By focusing only on individuals born one month before and after the invisible “cut-off” point determined by the administrative division between these two months, we should eliminate all potential “season of birth” effects, because children are born in the same meteorological and cultural “season”. In fact, also any influence deriving from parents with different characteristics targeting different birth timing should be reduced, because although it is possible to plan a period for the birth of a child, it is difficult to exactly plan a specific month. Therefore, children born close the cut-off should be more similar in terms of unobservable characteristics. The “discontinuity sample” test confirmed strongly the main result. Although the sample size reduced to one fifth of the original size, the estimated coefficient remained positive and significant, and, interestingly, we obtained a very similar estimation of the coefficient than the one obtained with the inclusion of the control for temperature at birth. In conclusion, our robustness tests showed that the results presented in this section are consistent even when “season of birth” effects are controlled for. Moreover, when we net out the effects of seasonality (columns 5 and 6 of Table 3), the estimated coefficient increased from 0.024 to 0.031.

Figure 3



Source: GSOM/GSOY (Global Summary of the Month/Global Summary of the Year) for the city of Milano. Available on line from NOAA National Centers for Environmental Information at: <http://www.ncdc.noaa.gov/cdo-web/> (Last access 26th October, 2016)

Heterogeneity analysis

In Table 4, we investigated the presence of heterogeneity in the association between retirement age and subsequent health. For this purpose, we have analysed the sample of retirees who were working after 1985. We have analysed how the estimated coefficient associated to pension age varied between retirees who, before retirement, were employed in manual/non manual occupations, had a low/high average wage, and were working in secondary/service sectors¹⁴.

¹⁴ The secondary sector was defined as manufacturing, constructions, transportations and mining activities. Low and high wage were defined according to the median.

Table 4 Heterogeneity analysis of the Effect of Retirement Age on CVD Hospitalization (TSLS)

	Low wage	High wage	Manual	Non Manual	Secondary	Services
	0.044**	0.014	0.041***	-0.002	0.028***	0.009
	(0.005,0.084)	(-0.003,0.031)	(0.017,0.065)	(-0.026,0.021)	(0.009,0.047)	(-0.023,0.042)
Instrument	Q1	Q1	Q1	Q1	Q1	Q1
F-test	23	138	69	54	105	28
Observations	25,427	25,427	34,400	16,454	34,694	15,646
CVD incidence	0.068	0.064	0.067	0.064	0.065	0.069

Notes: Each column represents separate regression. All regressions include year and region of birth dummy variables, log of wage, years of contribution and sector of activity dummies.

Robust 95% CI in parentheses. Legend: * $p < .10$, ** $p < .05$, *** $p < .01$

From Table 4 it emerges a clear socio-economic gradient of the effect of retirement age on health. Indeed, the overall detrimental effect of retiring one year later is mainly driven by the retirees who, during their career, were employed in low wage jobs, in manual occupations and in the secondary sector. Among these categories of workers, retirement at older age is associated to an even higher growth in the incidence of CVD hospitalization than the one found in the overall population, while for the other half of the sample, the TSLS procedure suggested a null effect of retirement age. Among the poorest half of the sample and manual workers, the estimated coefficients are similar. For these segments of the population, the increase in the incidence of CVD hospitalization was on the range of 4.1-4.4 percentage points, much higher than the average that we got in the full sample. In turn, when we stratified between sectors, the point estimate was similar to the effect found on the full sample ($\hat{\beta}_{secondary}^{IV}=0.028$, $p<0.01$), possibly because the “secondary sector” is a very broad level of disaggregation, which pools together manual and non-manual, as well as low and high wage earners. It is worth to notice that the instrument resulted relevant and monotonous in all the categories.

Discussion

The result of a positive effect of retirement age on the incidence of CVD hospitalization is in line with Bound et al. (2007)¹⁵ who, considering UK, found a significant reduction in the probability of CVD risk factors (excessive waist circumference, elevated triglyceride level, low levels of HDL cholesterol, high blood pressure, and elevated fasting blood glucose)¹⁶ after statutory pension age. However, our finding is in contrast with most of the previous literature investigating CVD outcomes (Bamia et al. 2008; Behncke 2012; Morris 1994; Moon et al. 2012; Dave et al. 2008). An important methodological difference that can explain this divergence in the results is the study design adopted to address the problem of endogeneity¹⁷. Bamia et al. (2008), Morris (1994), Moon et al. (2012) and Dave et al. (2008) did not adopt a quasi-natural experiment set-up and their preferred specification relied on the control for confounding factors at baseline. All these studies found that retirement is associated to higher probability of CVD, but the problem of endogeneity in the retirement choice was not addressed. Behncke (2012), who studied UK and adopted more than one methodology, found a higher risk of CVD associated to retirement when estimating a propensity score matching model, but the result turned to be null when she replicated the analysis instrumenting retirement with age-specific retirement incentives. Finally, the positive and significant effect established by Dave et al. (2008) among US retirees vanished when the analysis was constrained to the individuals with a health insurance before and after retirement.

15 Bound (2007) analysed in a RDD fashion the discontinuity in age-specific trend before and after 65 years old, the UK pension eligibility age for men in the period under study.

16 The presence of three out of the five cardiovascular diseases factors (excessive waist circumference, elevated triglyceride level, low levels of HDL cholesterol, high blood pressure, and elevated fasting blood glucose) is classified as metabolic syndrome (Bound et al. 2007).

17 Other differences are, for example, that for none of the mentioned studies the outcome considered was CVD hospitalization, but rather it was mortality for CVD (Bamia et al. 2012, Morris1994) and self-reported CVD (Moon et al.2012; Dave 2008, Behncke 2012). Apart from Bamia (2008), in all these studies retirement was operationalized as a binary treatment. Finally, these studies included also different reasons for retirement, beyond occupational retirement.

According to our results, the association between higher retirement age and CVD health deterioration varies substantially between different strata of the population. We showed a significant ($p < 0.01$) increase in CVD incidence associated to delayed retirement among blue-collar, low median wage and secondary sector employees, while among non-manual, high median wage and service sector workers retiring one year later had a null effect. Hence, the harmful consequences of higher pension age are closely linked to the “quality” and to the “quantity” of work exposures before retirement. Retirees exposed to more disadvantageous working conditions face higher CVD risks as a result of retiring at older age. Our findings are broadly speaking consistent with the vast literature on the socio-economic health inequalities (Brunner and Marmot 1999; Pollit et al. 2005) and point to conclusions similar to the studies of Westerlund (2009), Matthews (2014) and Kalousova et al. (2015). By using data from the Gazel cohort study (Westerlund et al 2009), SHARE (Kalousova et al. 2015) and the English Longitudinal Study of Ageing (Matthew 2014) these authors found that retirement is protective for health among workers in low quality jobs only, where quality of job was defined as jobs with high exposure to physical work or psychosocial factors. Mazzonna and Peracchi (2016) found a similar protective effect, though in the context of age related cognitive decline. Indeed, by exploiting pension eligibility variation between and within European countries, they showed that the negative effect of retirement on cognitive ability disappears when focusing on people who worked in more physically demanding occupations. For these individuals, retirement has an immediate beneficial effect on both self-rated health and cognitive abilities.

What emerged in our study suggests that potentially harmful occupational hazards transmit further beyond the borders of work, as they impair also future health through a continuity of exposures that combine, cumulate and interact throughout the work life (Blane 1999; Blane et al. 2013). Coherently with the behavioural and pathogenic pathways for CVD (Marmot et al. 1997; Brunner 1997; Brunner et al. 2006; Perk et al. 2012), the biological plausibility of our findings might origin in the psychosocial risk factors associated with work. High physical demands, low control over own work, irregular shifts, negative emotion of hostility, anger and isolation are all crucial psychosocial risk factors which can trigger unhealthy behavioural consequences (e.g. food choices, smoking, drinking, sedentary lifestyle, sleep disturbances). These behavioural reactions in turn are known to activate or accelerate pathogenic

mechanisms that are involved in cardiovascular diseases (e.g. autonomic dysfunction, sympathetic-adreno-medullary activation, inflammatory and homeostatic processes, hypothalamic pituitary-adrenocortical-activation, and metabolic dysfunction) (Perk et al. 2012, p.40). Hence, retirement might benefit individuals' health directly because of the "relief" from poor psychosocial work conditions, and indirectly through the engagement in healthier lifestyle, as suggested by recent empirical literature (Lang et al. 2007, Engberg et al. 2012, Eibnich 2015). For example, Eibnich by adopting an RDD design with GSOEP data, after showing a strong positive effect of retirement on several health indicators, investigated a set of possible underlying mechanisms. He found that this association is mediated by decrease smoking, increase in sleep duration, increase in physical activity (gardening and repairs) and relief from work-related strain. This is highly relevant to our study, since smoking, sedentary life, high weight, poor sleeping are all well-known modifiable risk factors for CVD, as suggested by international guidelines both in Europe (Perk et al. 2012) and USA (Lloyd-Jones et al. 2010).

5. Summary and conclusions

This article analysed the effect of pension age on the incidence of hospitalization for cardiovascular diseases at the age of 68-70 years, using a large administrative Italian database that combines archives on social security records and hospitalizations for workers of the private sector. We focused on the 1937-1944 cohorts of males who were employed or self-employed in the private sector and who retired via occupational retirement. We adopted an identification strategy based on instrumental variable to overcome the problem of endogeneity of retirement, exploiting the discontinuity in retirement age induced by birth month. To the best of our knowledge, this is the first time that age at occupational retirement is taken as the exposure variable. Moreover, month of birth is an unexplored instrument in this field; nevertheless, its applicability for addressing similar research questions is promising and deserves further investigations. Indeed, the instrument solves the problem of endogeneity of retirement in the absence of policy changes and when the assumption of monotonicity is warranted.

Our results point to a significant detrimental effect of postponing retirement on the insurgence of new coronary and stroke events. Delaying retirement by one year rises the

incidence of hospitalization at 68-70 years old by 2.4 percentage points ($p < 0.01$), holding constant years, region of birth and work-related characteristics before retirement, i.e. occupation, sector of activity, years of contribution at 52 years old and average wage. Such increase corresponds to a relative risk of about 1.36, comparable to the excess risk of acute coronary events found among those who smoke less than 10 cigarettes per day relative to never smokers (Mons et al. 2015). The result of a detrimental effect of higher pension age on CVD was robust to the possible confounding effects of climatic conditions at birth and of self-selection of mothers across the calendar year.

The detrimental effect of late retirement was even higher among retirees previously employed in manual occupations, in the secondary sector and with low wages ($\hat{\beta}_{low\ wage}^{IV} = 0.044$, $p < .05$; $\hat{\beta}_{manual}^{IV} = 0.041$, $p < .01$; $\hat{\beta}_{industry}^{IV} = 0.028$, $p < .01$). On the other hand, a null effect was found among non-manual workers, employed in services and in high paid jobs. These inequalities in the effect of extending working life on cardiovascular diseases suggest that the role played by the exposure to adverse physical and psychosocial risks throughout the work life is crucial. Moreover, the burden of ill health, which is usually stronger among older workers in demanding and less paid job positions, is further enhanced by postponing retirement.

In conclusion, when designing and evaluating the effects of a pension reform, whose intent is to prolong working life, the possible unintended effects on health should be always taken into account. This is important not only because of the intrinsic value that health has, but also because of its societal and economic costs, i.e. higher health care costs and increased expenditures in other social welfare programmes. A final consideration regards the distributional consequences of pension reforms that rise pension eligibility for all workers in the same way. According to our results, this type of policy produces health deterioration for more disadvantaged socio-economic groups. Thus it consolidates the well-known social gradient in health, while also contributing to generate regressive distributive mechanisms, as employees in less paying jobs, due to a worsening of their healthy life expectancy, will also receive shorter pension flows.

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